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31 March 2014

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Dear Editor,

Please find below the answers to the reviewers' comments. Overall, the reviewers' suggestions were very helpful, and we believe that including them in a revised manuscript would majorly improve the paper, and hopefully make it acceptable for publication on Biogeosciences.

We are looking forward to your answer,

Sincerely,

Research fellow & Lecturer
Department of Animal and Plant Sciences
The University of Sheffield, UK
Research Professor,
San Diego State University, CA, USA

Anonymous Referee #1

The authors compare growing season eddy covariance CO2 exchange (GPP, Reco, and NEE) between two years in Alaskan arctic wet sedge; one 'normal' year and one 'extreme' weather year (experiencing summer drought and warming). They also measure net Sphagnum CO2 exchange (NSE) and discuss the impact of drought and warming on moss vs vascular plants – and the ability of the ecosystem to adjust to such extreme weather events. This is an interesting and timely topic, and in general I find the manuscript well-written. Please find comments and concerns below.

We thank the reviewer for the positive evaluation of our manuscript. We will include the suggested corrections in the revised version of the manuscript.

First, I'm unsure if a 2 degree Celsius warmer growing season (relative to the long-term temperature mean) is really considered an 'extreme weather event'? Compare this with the temperature amplitude (and shorter duration) of the extreme warming events investigated by e.g. Stef Bokhorst and colleagues in Abisko, N Sweden. A 2 degree warmer growing season is more or less equivalent to the ITEX open-top chamber warming methodology – and the authors could include and discuss such references in more detail in this manuscript.

Our P-PET calculations show that summer 2007 presented the lowest summer rainfall, and more importantly, the largest summer water deficit throughout an entire 65 yr record. Overall, the unusual conditions were mostly linked to water deficit more than high temperature. We will emphasize this in the revised manuscript and we will also include the suggested papers in the discussion.

Second, I would like to see weather data (as presented in figs 1 and 2) from 2008, and in particular I would like to see water table and thaw depth data from 2006 and 2008. Such data could be put in the supplementary info (if data exist).

We will include this data in the revised manuscript.

Regarding CO2 data: Why not analyse multiple years (e.g. including 2005, 2008 and 2009) instead of just 2006 and 2007 (table 2)? This would convince your readers that 2007 really was an 'extreme' year – and how it affected CO2 exchange in 2008. Also, why is incomplete CO2 data presented from 2008 and 2009 (only cumulative net CO2 uptake is presented in the text)? How CO2 exchange responded, i.e. all CO2 compartments, in 2008 would seem to be a pretty big part of the story here. Likewise, 2009 data showing how the CO2 compartments were 'back to normal' would also seem to be a big part of the story.
Adding these years would significantly improve the manuscript.

We will include this data in the revised manuscript. The only dataset that was collected only in 2006 and 2007 is the moss photosynthesis, so unfortunately this could not be added to the paper. However, all other dataset were, can be easily included, and can address most of the concerns of the reviewer.

Third, the authors conveniently side-step discussing the impact a lowered water table and deepened soil thaw layer may have on the soil microbial community, affecting soil respiration and decomposition rates – which in-turn would change nutrient availability for the vegetation. As the authors seem to want to discuss their findings at the 'ecosystem' level, which I encourage, they should include the soil environment in their discussion. I would have liked to see data on soil nutrient pools and/or nutrient fluxes between the years in question, thus getting a more detailed view of the impact of the drought and warming on the whole ecosystem. However, I acknowledge that this would have been a much greater undertaking. At the very least, warming induced changes in soil respiration should be discussed in the paper. See e.g. Dorrepaal et al. 2009, Nature, for details on how warming may affect CO2 efflux in a subarctic peatland.

We will include soil pore water NH_4^+ for 2007-2009 and soil microbial biomass C (MBC) for 2006-2009. Both soil pore water NH_4^+ and soil microbial biomass C were very high in 2007 and then values declined in 2008 and 2009. The differences by year are all significant except MBC 2008 vs. 2009. So the warmer conditions of 2007 stimulated microbial activity and N release. This data and discussion will be included in the revised manuscript.

The results section is unusually long with lots of text. In comparison, the discussion section is quite short. I would suggest, shortening the results section text, and expanding on my concerns above in the discussion.

We will include these suggestions in the revised manuscript, shorter the results, and expand the discussion.

There are too many figures, e.g. figures 4 and 5 could be put in supplementary info. I am not fully convinced of the novelty of the hypotheses, and the authors should address this concern and make changes to the introduction accordingly. Convince the reader that the study and its findings are novel. Additionally, the hypotheses are not mentioned at all in the discussion.

We will include these suggestions in the revised manuscript, and rephrase the hypotheses to highlight the novelty of this study.

Please find comments/edits to the text below. I hope they may be of use for the authors.

We will include all the corrections listed below in the revised manuscript.

19191 Lines 1: What do you mean by “mode of action”? Please rephrase.

We will specify this in the revised manuscript.

Line 9: Here, you call your study site an “arctic tundra ecosystem”, which may be true but that could be anything from polar desert to fen ecosystems. Please be specific – throughout the manuscript text.
Line 16: Insert ‘warm and dry’ after ‘extremely’.

We will specify this in the revised manuscript.

Line 21: Consider changing ‘uptake’ to ‘sink’.

We will change this in the revised manuscript.

19192 Lines 1: Insert ‘in’ after ‘increases’.

We will replace this in the revised manuscript.

Line 9: What does this relate to; ‘Drought and extreme temperatures are the most important extreme events to understand’?
More important than what? Consider rephrasing to: ‘Extreme weather events such as drought and (extreme) warming are hugely important to understand: : :..’

We will rephrase this in the revised manuscript.

Line 16 and throughout the manuscript text: Why not call it ‘warming’ instead of ‘temperature increase’?

We will change this in the revised manuscript.

Line 25: Change to ‘and/or drying’

We will change this in the revised manuscript.

Line 27: Warming also increases deep peatland soil carbon release to the atmosphere, Dorrepaal et al. 2009, Nature.

We will mention this process in the revised manuscript.

19193 Line 3: Either be more specific than just saying ‘arctic tundra’ - or at least add ‘ecosystems’ after ‘arctic tundra’.

We will include ecosystem this in the revised manuscript.

Line 5: Insert ‘regime’ after ‘precipitation’ Line 14: Specify what kind of tundra ecosystem.

We will include this in the revised manuscript.

Line 14: From reading the introduction, I’m not convinced of the novelty in hypothesis (i).

The fact that even these high latitudes system can experience drought stress is not common, so this hypothesis is novel considering the ecosystem.

Line 16: How is hypothesis (ii) really different from (i)? Line 16 and throughout the manuscript text: Always call it ‘net C uptake’, b consistent.

We will couple the first and second hypothesis.

Line 19: Specify that you are comparing upland NEE to plot-scale moss measurements

We will specify this in the revised manuscript.

19194 Line 13: From what I can gather you only used CO2 data from one tower. Delete the first sentence of this paragraph, and start it with ‘Three EC towers: : :.’

We will remove this first sentence in the revised manuscript.

Line 18: ‘Increased’ not ‘increase’.

We will change this in the revised manuscript.

Line 20: The Zona et al. 2009 reference is not apparent in the references which only list Zona et al. 2011 and 2012.

We will include this in the revised manuscript.

19195 Line 6: delete ‘in’ before ‘Lasslop’.

We will remove this in the revised manuscript.

Line 19: “experienced’ not ‘experience’.

We will change this in the revised manuscript.

19196 Line 7: why 2-4 cm and not just 4 cm every time? I would be concerned that differences in soil moisture could be attributed to e.g. comparing moisture from a larger moss biomass at one date to moisture from a smaller moss biomass. This would be similar for the NSE measurements – but here you always used 2 cm, avoiding the issue.

The moss surface was sometimes very variable, so this was just the natural variation of the top of the moss layer within each sampling plot for the water table (the samples were bigger than the ones used for the photosynthesis measurements). We will mention this in the revised manuscript.

Line 8: 'date' not 'dates', insert comma after 'date', delete 'and' after comma, insert comma after 'cans'

We will change this in the revised manuscript.

Line 9: Consider removing the sentence starting with 'Sphagnum water content: : :'.

We will remove this in the revised manuscript.

Line 11: So you removed 8 moss samples per plot? If so, add 'In each of' before 'three different plots'. Also, this is the first time said plots are mentioned in the text and I would like to see more info on these; e.g. specify plot size, average distance between plots, and average distance between samples inside each plot.

We will add more details in the revised manuscript.

19197 Line 2: What biomass samples? Line 9: add 'and' before 'LAI'.

We will add this in the revised manuscript.

19198: Line 12: What is 'VPD'? It hasn't been stated yet.

We will define VPD in the revised manuscript.

19199 Line 19: Be specific, what kind of arctic tundra ecosystem?

We will add more details in the revised manuscript.

19201 Line: 18: Delete 'may'.

We will delete this in the revised manuscript.

19202 Lines 4-6: Allow me to be blunt and ask 'so what?' – Instead of starting the discussion by saying that you found drought to affect GPP and Reco as also found in other studies, I would start with the most novel finding of this study, enticing the reader to read on.

We will remove this part and better highlight the novelty of this study in the revised manuscript.

Figure 3: Is it realistic that NSE would equal Reco on 12-30 June in 2007?

We will include all the listed comments in the revised manuscript.

Anonymous Referee #2

Received and published: 21 January 2014

General comments

The authors present an interesting data set on the land-atmosphere exchange of CO2 from the Arctic tundra in Barrow, Alaska. The summer in 2007 was unusually warm and dry, but the CO2 uptake was not lower than usual. On the contrary, during the normal summer of 2008, from a meteorological point of view, the CO2 uptake was lowered. The authors suggest that the ecosystem had difficulties in readjusting to normal conditions after an extreme year.

We thank the reviewer for the positive evaluation of our study.

In its present form, the paper is too speculative. The data and analyses do not satisfactorily back the conclusion that the ecosystem "momentarily lost its low-temperature acclimation". Alternative interpretations are possible; such as a high insolation in 2007 (table 1) that may have stimulated vascular plant activity (not discussed in the paper), while Sphagnum mosses suffered from desiccation.

We monitored vascular plant activity by measuring LAI, which did not show an increase (this was discussed in the manuscript). We also compared the light response curve of 2007 and the other 4 years (Fig. 6). We will include more careful discussion on this point in the revised manuscript, and remove speculations.

In 2008, during more normal conditions, Sphagnum activity were reduced. High GPP in 2007 was not reflected in the LAI measurements. This may indicate that vascular plants invested more than usual in root production to cope with decreasing groundwater tables in 2007. In 2008, the larger than usual belowground plant C pool resulted in higher rates of autotrophic respiration. However, on the basis of data and analyses presented in the ms it is hard to evaluate both the authors' hypotheses and the alternative ones. Most of the analyses are conducted for 2006 and 2007. Information on GPP, Reco and NSE is not presented for 2008 and 2009. Light response curves are shown for all years but these suffer from having too long periods of data (see below) preventing meaningful interpretation. In addition, the flux partitioning method performed in this study is based on Reichstein et al. (2005), which requires dark periods for successful application. Due to the high latitude location of Barrow dark conditions should be absent for at least the first half of the measurement period in each year, leading to uncertain estimates of GPP and Reco.

Unfortunately we did not collect NSE in 2008 and 2009, so this dataset cannot be include, but we will include GPP, and Reco for all the five years of data in the revised manuscript. We will also investigate if the light response curve change depending on the time of the season. The reviewer rightfully highlighted that the partitioning method might be problematic in this high latitude ecosystems. However, we cross compared this to actual data from chambers

Detailed comments:

Abstract

Authors discuss C uptake but this paper only deals with CO2 exchange and not other important flows of C such as CH4 exchange and C runoff. Suggest rephrasing "C" to "CO2" throughout the manuscript.

We will modify this in the revised manuscript.

Materials and methods

P19194: L13-22: Please include basic information on the EC system such as type of gas analyzer, anemometer and height of measurements.

We will include these details.

P19195: L3-6: Flux partitioning was based on Reichstein et al., 2005. However, this method requires nighttime periods to be able to produce reliable estimates of Reco and hence GPP. The latitude of the site (71 deg N) is likely to have midnight sun throughout a large part of the study period (June-August) in each year. A more adequate method would be using light response curves to partition NEE, see Runkle et al. 2013 Biogeosciences 10, 1337-1349.

This is a good suggestions, and we will mention this method in the revised manuscript.

Reults
P19199, L5-6 and table 2: Authors state that GPP and Reco were significantly more positive in 2007 compared to 2006; however no post hoc tests on the difference have been performed

A post-doc test was performed, we will mention this in the revised manuscript.

P19199, L17-20: This part belongs to discussions section

P19199, L20 (and other places): Desiccation is a more usual term used within the literature on Sphagnum compared with necrosis. Can the authors be sure that Sphagnum actually died during the dry period?

We will change this term in the revised manuscript.

P19200, L5: Please indicate average (and st.dev.) of LAI for 2006 and 2007 (preferably for 2008 and 2009 as well).

We will include averages and st. deviations in the revised manuscript.

P19200, L23-P19201, L2 and Fig. 6: The light response curves cover too long period including pre-leaf, peak season and post-leaf periods. Thus the information gained from Fig. 6 is scattered and limited due to seasonal evolution in vegetation greenness. It would be better to divide the measurement season into 10-20 day periods, such as the periods in Fig. 3, and to display four sets of light response curves.

We will separate the light –response curves in the revised manuscript, into different periods and investigate differences in the response of the ecosystems in different times of the season.

Discussion
P19201, L19-P19202, L5: This text belongs to Introduction section.

We will move this to the introduction.

P19202, L21-22: I am not convinced that the ecosystem momentarily lost its "low temperature acclimation". Vegetation is generally adapted to its long-term environmental characteristics, cf Yuan et al., 2011, Biogeosciences 8, 1453-1463, and one extreme year would not be enough to change the acclimation.

We will modify the discussion in the revised manuscript, and tune down this statement.

Tables

Table 2., caption last sentence: Replace "dysplaied" with "displayed"

We will include this correction in the revised manuscript.